Interview of John McGowan
May 18, 2012

PETER BRUEGGEMAN: Today is May 18, 2012. I am Peter Brueggeman with the Scripps Institution of Oceanography archives. I am here with John McGowan and Paul Dayton.

JOHN McGOWAN: My name is John McGowan. I am an emeritus professor of Oceanography.

PAUL DAYTON: I'm Paul Dayton, and I am at Scripps too. I am also a emeritus.

PETER BRUEGGEMAN: As I mentioned, the Scripps archives has an oral history of you conducted in 2006, and there is a lot of great information there, not to be overlooked. Paul and I were talking about Scripps, and there are some areas that Paul was particularly interested in ferreting out.

PAUL DAYTON: I just think that you have a really fascinating history. Instead of just focusing on the Scripps stuff, I was much more interested in focusing on you and going back to your early life. I think some people in your cohort represent just a wonderful history of Americana as well as science. Yesterday, you were telling me a little bit about your preschool and school years in Wisconsin, which I think are really interesting.

JOHN McGOWAN: Well, I came from a small town in northern Wisconsin, Oshkosh. It was right next to a very large lake, a beautiful lake. We lived outside of town in a cottage. There were other cottages nearby that were just occupied during the summer time. Our cottage was uninsulated. Wisconsin winters were pretty rough. I do remember insulating around the base of the house with a lot of leaves. That was still a remnant of the old hardwood forest… lots of maple and oak, willow. We piled three or four feet of leaves around the base of the house, and the house was heated by an oil stove. My dad cut radiator fins out of old soup cans and tied them around the stove pipe so it would radiate more heat. My brother and I slept upstairs in an unheated room, except the chimney went through the room. So there was a little warmth. I do remember waking up in the morning with frost on the blankets and moisture coming from our own breath. But it was fun. It was particularly a good place to live because in the spring, when the snow melted, there were lots of vernal ponds. These pools after the first few warm days of spring, really bloomed, and my first experience with aquatic biology was laying on

my belly staring into the ponds looking at all the little bugs and algae that were swimming around.

PETER BRUEGGEMAN: Do you remember what age you were when you first really got interested in looking at the aquatic life there, up close?

JOHN McGOWAN: I would say I was in sixth or seventh grade. Of course there was a lake itself and there were algal blooms, and all kinds of wonderful things happening in the water. In the winter, there are some places where the ice was so clear, that you could lay on your belly on the ice and stare down into the lake as if it were a glass bottom boat. Even in the winter time there were lots of organisms down there. I spent some time doing that. Of course, I tramped through the woods a lot, remnants of the old woods, and just in general spent a lot of time observing nature.

PETER BRUEGGEMAN: Did you collect any animals, with jars?

JOHN McGOWAN: No. I was not a collector. I would bring stuff home and my mother would encourage me to. She was very excited about anything I brought home or encourage me to go do more. So she had a lot to do with generating my interest in natural history and ecology, although the word wasn't used in those days. During the great depression, our family was hit hard. Many families were, almost all of them were in that town anyway. My father inherited some insurance money from the death of my grandmother, his mother, and we used that money to move to California. The theory being it is much better to starve to death in a warm climate than a cold one. Things weren't much better in California, financially but I did start high school --

PAUL DAYTON: Where did you live?

JOHN McGOWAN: In Alhambra, because my mother's brother was in Alhambra. We picked that for no other reason. Neither of my parents ever attended high school, and I never learned to study. And as near as I can remember, I didn't do one single night of homework, not once. I did learn after a while, that you were supposed to take notes, and I didn't know quite what to take. I also remember my mind wandering during lectures. First two or three sentences out of the teacher's mouth usually set me off on some wild diversion, distraction. I simply didn't hear anything else.

PETER BRUEGGEMAN: Would you say you are more of a visual person then?

JOHN McGOWAN: Yeah, it's terrible. They make one or two statements, and I start thinking about what they said, and I don't hear anything else. It still bothers me today, lots of lectures and seminars I go to at Scripps.

PAUL DAYTON: So you are not doing so well in high school…

JOHN McGOWAN: I did very badly in high school, very poorly. I never graduated. I never made enough credits. I never got a high school degree. Neither did my parents, and they didn't know enough about academic matters to be worried about my failure. It never occurred to them to say, "Don't you have some homework?" So I didn't do any, ever. Not once.

PAUL DAYTON: So you were still sort of a high school student, and the war was starting.

JOHN McGOWAN: Well, the war started in 1942 -- ctually, late 1941, in December, and I was a senior at the time, and as I say, I was a very poor student. I was also distracted. We moved around Alhambra five or six times during my high school career, three years, and that winter, my brother who was a fighter pilot in the Army Air Corps was killed. That distracted me quite a bit, so that was a very bad year for me.

PAUL DAYTON: Was he killed in the Pacific or the Atlantic?

JOHN McGOWAN: The Atlantic. We were all about to be drafted anyway, all the boys. We were all 17 or 18. They started drafting kids at 18. So there wasn't much of a future other than being in the military. I joined in with my classmates, four of my classmates, and we joined the Navy at age 17. We went down -- a group of us went down to the recruiting station in downtown Los Angeles. There was a Marine recruiter on one side and the Navy on the other, and I said what the hell, let's join the Marines. I like those uniforms. One of my friends who had better judgment said, "No, my father was in the Navy. I want to join the Navy." So I said, okay. I came down here to San Diego in the Naval Training Station, and about a third of my boot camp class went on to schools, radio operator schools, radar schools, motor mechanics. I remember they gave us a test, an aptitude test for radio Morse code, and I purposely flunked it because I was damned if I was going to be a radio operator. I wanted to be in the Navy, shoot guns, so that's what happened.

PETER BRUEGGEMAN: Were you aware of Scripps Institution at all when you were here in San Diego.

JOHN McGOWAN: No. Not at that time. I was 17. It was 1942. I was very much aware of marine biology and was interested in it even then, because I had a high school teacher who was a bit of an old maid, fuss budget, but she did know something about marine science, and she had taken us on a couple of field trips to Long Beach.

PETER BRUEGGEMAN: San Pedro, to the tide pools there?

JOHN McGOWAN: Yes, the tide pools at San Pedro and the mud flat. It turned out the mud flat was far more interesting. There was a lot of stuff growing there at that time. I wouldn't say she was an inspiring teacher, but I was preconditioned to be very, very interested. During the war, I was on an ammunition ship for two years, and that ship would anchor in some god-awful stinking little island in the South Pacific and act as a floating ammunition depot for the fighting ships nearby. That meant that we stayed two, three or four weeks in one particular little harbor. I got designated to be a boat operator to haul ammunition from our ships to them to unload. There are lots of coral reefs nearby, and I spotted those right away. We would take off a little extra time and go diving around coral reefs.

PAUL DAYTON: You had masks?

JOHN McGOWAN: No. Masks had not been invented, but goggles. They weren't diving goggles. One of the things enlisted men do, deck apes do in the Navy is chip paint, and they provided us with paint chipping goggles that had lots of little air holes in them. I got a pair of those goggles and filled up the air holes with beeswax so that I could dive. They would keep the water out for maybe three or four minutes, enough to get a good look.

PETER BRUEGGEMAN: You didn't have equalization pressure problems with them over your eyes?

JOHN McGOWAN: No. I didn't go that deep.

PETER BRUEGGEMAN: You were diving down under the water?

JOHN McGOWAN: Yeah, maybe ten feet at the most, and that was enough

to see a lot of beautiful, beautiful coral, totally untouched. Especially in what is now called Vanuatu, the New Hebrides, Efate and Espiritu Santo and another island whose name I forgotten. We spent months there.

That was when I had one of my most frightening experiences. We would be anchored as a floating ammunition depot for maybe a month or two, and life was incredibly boring, except on those few occasions when we would get an air-raid. On Sunday afternoon, they often would have a movie down below deck in one of the holds that were emptied of ammunition, a Hollywood movie. The ship was crowded, and we all couldn't have seats. There was a row of chairs in the front row, that was officer country, but they allowed us to lay on deck in front of them to watch the movie, and then another hundred or so sailors behind them on benches. I remember one movie -- this was two decks down, and during one movie, there was a large bang, and a large whooshing noise, very loud. Of course, everybody thought that was ammunitions going. So it was a big roar, and everyone headed for the ladder. Well, there is only one little narrow ladder down into this hold, and about 200 guys going to scramble for it, and I was laying in front of it. I thought well, this is it, they are going to trample me to death. I jumped up and hid behind a big heavy stanchion, and I didn't even try and get out. I thought at the time, well, this is how it is going to be, this is the end. Some smart guy turned on the lights, and that stopped everything, right there. What had happened is that these holds all had fire suppressing apparatus in them, big CO2 tanks, and one of the CO2 tanks had blown, popped its lid, and there was a lot of whooshing noise of the CO2 escaping, which was bad enough. That calmed everything down. Everyone stopped dead, but it scared the hell out of me.

PAUL DAYTON: As I recall, you had stories of picking up planes, float planes?

JOHN McGOWAN: I was proud of that, that was on a cruiser. I got transferred from this ammunition ship to something called new construction. By then I was an old salt, you know, I had two years in.

PAUL DAYTON: By that time you were an old salt at 19?

JOHN McGOWAN: Yeah, highly experienced, combat veteran, and the Navy was building new ships as fast as possible to build them, and they had to staff those ships. They would take a cadre of experienced sailors around which to build a new crew, mostly young recruits. Well, I was put on one of those cadres and taken off my ship in the South Pacific and sent back to Newport, Rhode Island, to form a ship, be one of the

persons forming a ship company. Then we were assigned to a light cruiser, brand new one that was being put into commission in the Boston Navy yard. I had a lot of nice liberty in Boston and New York, and this cruiser was formed. I was the designated bosun's mate in the second division, back aft. In those days, light cruisers had sea planes, big ones, amphibious planes which they launched from catapults on the stern, and of course they had to recover them at sea. That was a tricky operation. Well, luckily, previously on the ammunition ship, I learned a lot of what is called marlinspikes seamanship, rigging blocks and tackles and splicing, and running lines back and forth and lifting heavy weights, dangerous heavy weights like 500-pound bombs. I knew a lot about it, so I was put in charge of sea plane recovery. I was 20, and that operation was really interesting. These planes were used for spotting during battle and reconnaissance, and God knows what. They also had little bombs on them that they would drop occasionally. They launched them with these big catapults. The catapult was probably 50 feet long, maybe more, and the plane would sit in a little cradle on a track. It was hydraulically operated, and the energy came from a 6-inch powder shell. It had a breach, and we loaded it with 6-inch powder shell… 2 and a half, 3 feet high, 6-inches in diameter, a lot of black powder. Put one in the breach, and the pilot would rev up the engine and finally wave to us. We would shoot it off. Kaboom. They always made it, as far as I know, but recovery was difficult. The recovery was done at sea while we were underway, and the ship would make a big circle to make a slick.

PAUL DAYTON: Calm the seas.

JOHN McGOWAN: Yeah, calm the seas down. The pilot would land, and we would crank out catapults that were projecting out over the side, and then drop down what is called a sea sled. It was a big heavy mesh rope cargo net, and it was towed on the surface. The pilot would taxi up to it and then kill the engine and fall back. He had a hook in the bottom of his pontoon, and it would hook in that sled. Then we had to move the crane over, move him in toward the ship, without hitting the ship with the plane, and then drop a hook down from our crane. The pilot would climb out of the cockpit and hook this in a big bridle and heave it up and move it over and drop it down in the cradle. Well, I was in charge of the operation. We had an officer who was a nice guy, but he didn't know a damn thing. And we had a chief bosun's mate who was just an old big ignoramus… dangerous. He was smart enough to keep his mouth shut. I was very, very much concerned about saving the pilot because of my brother. I saw my brother every single time those guys landed, and I wanted to do a good job and we did.

PAUL DAYTON: You did this in battle in the Pacific?

JOHN McGOWAN: Yeah, well, tried not to in the heat of battle. That cruiser was used primarily as anti-aircraft defense in protecting our big carriers. We cruised around. It was Task Force 38. Two or three, or sometimes four, really big carriers. They were bombarding the hell out of us out there in Japan, and of course the Japanese had sent out waves of kamikazes. So there were battleships and cruisers, and destroyers and rings, concentric rings around the carriers to provide anti-aircraft protection. Occasionally, we would be detached and sent within a mile or two of the coast of Japan to bombard something or other, targets, in preparation for landing and invasion. I was also a gun captain on a 6-inch gun inside a turret, and the second most frightening experience in the war was we were firing at something. I don't know, they never told us. You go to general quarters and man your gun and you start firing, and then you stop. You don't know what the hell is going on. You are inside a turret.

PETER BRUEGGEMAN: Because you are firing by coordinates, right?

JOHN McGOWAN: You are firing by coordinates. You get all of the information from the bridge up above, from the gun boss and his crew, and they tell you turn here, turn there, commence firing. Well, as gun captain, I was on the left side of this big breach of a 6-inch gun, and I had what looked like a little gear shift knob. I could open the breach and close it, and it was my job when we started firing, to open the breach. Then there were some ammunition handlers on the right-hand side of the gun, big strong guys. They wanted to pick up a 6-inch shell and dump it in this cradle, and then I would open the breach and ram that shell up into the gun. I retract the hydraulically driven cradle, and they would dump a big 6-inch powder charge in there, and I would ram that home, and close it, and then signal ready. Somewhere, somebody would press a button and fire the gun. There were three guns in that turret, and all three were firing at once and in salvos. One time my gun didn't go off. It was a hang fire, something wrong with the fuse in the 6-inch shell. Normally, I learned later, the Navy protocol is you wait an hour before you unload because it could be a slow acting fuse or it could all of a sudden fire or something. We got the order to unload immediately, so they evacuated the whole turret except for my gun. I must say, the Ivy League, pleasant but dumb turret officer stayed in the turret, and that was brave of him. The turret captain, who was a chief gunner's mate, knew what the hell he was doing, thank God for him. So we were told to unload, and he clambered up over the gun, and he had a big tool that he opened the breach by hand. Normally, it opens up after firing automatically. He then eased the powder charge out. My shell man, powder man,

Pete Bernarick (sp?) I'll never forget Pete, a big Polish boy from Pennsylvania, and strong as an ox. He picked up this powder charge and scuddled his way back and handed it over, all that apparatus to me. I held it like this and scuddled my way back towards the rear of the turret, and handed it to the turret captain, who handed it out of a little port to a guy on deck, and they threw it overboard. It could have gone off at any time. An explosion like that inside a turret would be absolutely nothing left of anything. That was scary.

PAUL DAYTON: So you didn't see what you were shooting at?

JOHN McGOWAN: Well, much of the time, we bombarded the coast. They were potential targets or, I think they were trying to fake out the Japanese because they knew a landing was imminent. The Japanese knew it would be imminent. First here. First there. So we were at sea, that task Force 38 was at sea 115 days. We were supplied at sea with tankers and refrigerator ships and what not. Back and forth up and down the coast of Japan. The big B-29 fire raids had been a failure because they couldn't hit any targets. All they could do is hit a city the size of Tokyo. So the Japanese were continuing to build kamikazes and buzz bombs and all kinds of nasty things. Production had not been slowed at all. So the Navy took over with precision dive bombings, and we were protecting the carriers doing all the dive bombings. They moved in towards the coast, close to the coast, and once in a while, we would be detached to bombard something or other. Then they dropped the A bomb, and every single man on that ship, including me, believes it saved our lives.

PAUL DAYTON: It saved your life because you otherwise were about to become an invasion fleet?

JOHN McGOWAN: Absolutely. No doubt about it. Somewhere, the order came down, no doubt from some bureaucrat in Washington, that you have already got 100,000 men out there just off the coast of Japan, why do you need to send more soldiers? Why do you need to wait? Why don't you invade Japan now? Well, 140,000 men were all the sailors on the ships, Marines, Army. And so, they started forming Marine Corps style landing groups, and I was in one of them. I was a bosun mate at the time so I was designated a BAR man, Browning automatic rifle, which was damn near as big as I was, but I did learn to fire it. We would have been a platoon, a landing force. Each and every ship had its own contingent of pseudo Marines, sailors who didn't know a damn thing about guns. Mess cooks and radar men and navigators.

PAUL DAYTON: And then Japan surrendered, and you were in the bay?

JOHN McGOWAN: Later, after the surrender. It was announced aboard ship that the bomb had been dropped. We knew that the war was coming to an end anyway before that. We were ready to invade Japan. They were going to lose and we were going to win, but the bomb stopped all of that. Then there was a hiatus of a few weeks, as I remember it, and the announcement was the Japanese had surrendered. So this task group was already just a few miles off Tokyo, and we sailed in Tokyo harbor, with the Missouri. We anchored in a row in Tokyo harbor. The Missouri was over there, we could see it, and somebody said, well, they are going to have surrender ceremonies today. Everybody aboard ship said, oh, that's nice. We were Exhausted, there was no big celebration. We were on watch, four on, four off, for weeks. Never got enough sleep, always this tension. We were anchored maybe a half mile away from the Missouri during the surrender ceremony. Then the announcement came, and we were headed back to Portland, Oregon for victory celebration. Everybody said, well, that's nice, we are going home. There was the biggest celebration, sailing up past Astoria. We were with a light aircraft carrier and two destroyers and us. The neat thing was when we went by Astoria the captain all got us out on deck and lined up along the rail. They had a high school band there, and that was it. That happened in Portland too, but there was no parade or no big celebration. I was 21 by then, due to be discharged anyway. The ship sailed down to Long Beach, and there was a center, they were just releasing sailors all along. It was just a pro forma procedure in this Navy center, and I got released in Long Beach.

PAUL DAYTON: You were just 21, and you hadn't graduated from high school.

JOHN McGOWAN: That's right. I hitchhiked up to Alhambra and tried to look up some of my old friends. A couple of them were around, and we went out and had a few beers. I decided well, I guess I have to go to work. There were job placement centers around, and, of course, nothing was too good for the boys. They got me a job in an adobe brick yard, and I was making adobe bricks and carousing around, bar crawling at night. After a few months of that, I decided I can do better than this.

PAUL DAYTON: Were your parents proud of you?

JOHN McGOWAN: Oh sure.

PAUL DAYTON: Did they come see you? They were glad you were alive.

JOHN McGOWAN: Yeah, yeah, that's right. They had a very small modest comfortable place in San Gabriel, which was a nice little town. So I thought, well, I guess I better get educated, so I went to Pasadena Junior College and explained it to a very wise lady counselor. She knew exactly what to do and make up my high school deficiencies, which I did, and did quite well. By then I decided I had to do some homework. Then, after a year and a half at Pasadena, I decided to go on. I was still uncertain of my own ability in a real university, so I was afraid to go to Berkeley or Stanford, although, I think I could have gotten in because I had very good grades at Pasadena. Furthermore, I liked the woods. I didn't know what a biologist could do for a living, and I wanted to do biology.

PETER BRUEGGEMAN: So at the Pasadena time you were thinking of biology?

JOHN McGOWAN: Oh yes, absolutely certain of it.

PAUL DAYTON: So now you are in Los Angeles wanting to do biology, but what makes you think of the woods?

JOHN McGOWAN: My earlier experience in Wisconsin, I like trees. I browsed through a bunch of catalogs in the library at Pasadena, and they had a good collection, and I spent a lot of time reading them. I noticed that Oregon State had a major in training guys for fish and game jobs. I thought, well, that is a way to earn a living as a biologist, so I applied and got accepted. Of course there was a GI bill and that changed everything, made it all possible. I visited Corvalis by bus. It is a nice pleasant, at the time, little town, and lots of big beautiful trees around. So I decided to apply there, and I got accepted. I took one semester of their fish and game courses, and they were so dumb, and so disappointing, and so dull, that I switched to zoology. I wanted to major in zoology.

PAUL DAYTON: What year was this?

JOHN McGOWAN: 1946 or 1947.

PETER BRUEGGEMAN: You went to the junior college for a year and a half, after the war ended, … it could be a little later.

JOHN McGOWAN: I am fuzzy on these dates. I was particularly interested in invertebrate zoology. I knew there was no way in hell you get a job doing that, but I didn't care by then.

PETER BRUEGGEMAN: And how were you interested in invertebrate zoology?

JOHN McGOWAN: I have no idea, I just was, because of the tide pools and my experience in the Pacific looking at coral reefs, and all of the strange critters that lived there. The crustaceans, and worms.

PAUL DAYTON: But now you are in Corvalis, which isn't on the coast.

JOHN McGOWAN: That's right, but they were in the process of setting up a marine laboratory at Charleston. There were some old CCC buildings there, and the university had inherited them. They were just barracks.

PETER BRUEGGEMAN: The Charleston laboratory which is now with the University of Oregon, and not with Oregon State University?

JOHN McGOWAN: Correct, actually it was in an Oregon State higher education organization, but Oregon State had taken it over, because Ivan Pratt, the invertebrate zoologist at Corvalis wanted it, and he talked to the Dean of Science into giving him a little money so they could go down there and have classes. They did that on a temporary basis, and then they decided to do it and have a summer station there.

PAUL DAYTON: And is it at the current location in Charleston, same spot?

JOHN McGOWAN: Yeah.

PETER BRUEGGEMAN: Were there salmon running in the little stream there?

JOHN McGOWAN: No, that little stream was built much later. Man-made by Jerry Rudy, who had taken over the directorship long after I left. Jerry, was the best thing that ever happened at OIMB, the Oregon Institute of Marine Biology. It was in a wonderful location, a little town in Charleston, a real old fashioned fishing town. Lots of fun, and beautiful tide pools and mud flats all over the place that had not been heavily impacted by man.

PAUL DAYTON: And this was 1947, 1948?

JOHN McGOWAN: Yeah. 1948, 1949. Yeah.

PAUL DAYTON: And I was, as a kid, not many miles away but living in logging camps between Prospect and Umpqua. John Isaacs was up there then, doing safety inspections of logging camp operations. He was working for the government when there would be some sort of fatality in the logging camp, he would go investigate it. He investigated the time when the choke setter in our camp was killed. John was there and he remembered it when we talked about it. I even remember as a little kid, this tall blonde guy walking around with all the loggers afraid of him. So then you went through Oregon State for your B.S.?

JOHN McGOWAN: And then I got a Masters there, I think I got my Masters degree in 1950. Maybe it was a Bachelors. Anyway, it was around 1950, 1951. That's when I came down to Scripps.

PAUL DAYTON: What got you to Scripps?

JOHN McGOWAN: Well, that was sheer accident, I had no idea where to go. I applied to several places by postcard, for God's sake, for graduate study. I didn't know any better. I didn't get any good advice on that, but I heard from a temporary instructor at Oregon Institute of Marine Biology that Scripps had been starting a new program on plankton and fish, and fish larvae, and it was called California Cooperative Oceanic Fisheries Investigation. It had just gotten started in 1949, and this was 1951. He told me who to write to, Martin Johnson, one of the authors of The Oceans, a book I never heard of. He said why don't you apply there. So I wrote Martin Johnson. Johnson had just gotten a bunch of money through MLR, to hire graduate students, and he said sure, come on out. So I had an assistantship, I was accepted at Scripps and I had a GI bill, but I was not a paid assistant, not for a year.

PAUL DAYTON: So this was in say, 1952.

JOHN McGOWAN: I think the winter of 1951, 1952 I think.

PAUL DAYTON: So you just got on a bus in Corvallis and came down here?

JOHN McGOWAN: Yeah, my parents were living in San Gabriel, so I came down and visited them. Then I bought an old Dodge 1937, and I drove down here and found a place to live, a room, and started graduate work at Scripps.

PETER BRUEGGEMAN: What was your Masters in, because obviously Johnson must have liked what you were doing.

JOHN McGOWAN: Yeah. I had done some reproductive studies on a local nudibranch. It had to do with the mechanisms of fertilizations and development. Actually, I was interested in larval development, but it was very difficult to keep the larvae alive it turns out, with the equipment we had anyway. I worked out their spawning behavior, and they are hermaphrodites. They switch sexes. The young ones are males and the older ones are females. So there is a tricky sex change going on, and they had a sperm storage organ -- so they copulate and exchange sperm, both of them, and store the sperm until their eggs matured. Then in a matter of weeks, they fertilize the eggs and lay a big egg mass.

PETER BRUEGGEMAN: So did you talk to Martin Johnson on the telephone? Did he interview you?

JOHN McGOWAN: No.

PETER BRUEGGEMAN: Was it strictly correspondence driven?

JOHN McGOWAN: Yeah, one letter each.

PETER BRUEGGEMAN: Well, things have changed. One letter. With recommendations, I assume from your professors?

JOHN McGOWAN: Oh, yeah. Sure, that must have been, but I don't remember.

PAUL DAYTON: Was Ivan Pratt your major adviser for your masters?

JOHN McGOWAN: Yes. Nice old guy.

PAUL DAYTON: Yeah. I knew him.

JOHN McGOWAN: Very nice guy, he actually was a parasitologist.

PAUL DAYTON: That's right. He wrote a textbook or something. Okay, so we get you down here. You meet Martin Johnson.

JOHN McGOWAN: I meet Martin Johnson.

PAUL DAYTON: And he lived in T-29.

JOHN McGOWAN: Yes, it was his home. Yes, the Martin Johnson house. He was a very quiet conservative, nice, man. He was not dynamic, however. Pretty conservative. I'm not sure if he was politically conservative, but a conservative person.

PETER BRUEGGEMAN: You mean quiet and soft spoken?

JOHN McGOWAN: Very much so. He had trouble articulating his thoughts. Well, you know, he had a lot of hidden qualities. He was very much -- yeah, taciturn is a word, Scandinavian. He didn't have much to say about anything, ever. There was a time that he invited not just me, but three other graduate students up for a party. At the time the liquor stores in town were all pushing a little kit, to make cocktails, and they were called Moscow mules. You could buy kits, and it was vodka and Kahlua in certain proportions in brass copper cups. So he bought one of these kits, and he made Moscow mules for us, much to his wife's dismay. She was very, very unhappy about it. We all sat around drinking Moscow mules, and he got quite tipsy, and he played his mandolin for us. He was pretty good. And more than that, he was a good cartoonist. He made some very, very clever cartoons. They were well done, and really quite funny.

PAUL DAYTON: But let's get back, Martin Johnson is an adviser and suddenly you go off to Micronesia. You want to talk about that.

JOHN McGOWAN: Well, I already started my thesis. I had gone through the basic courses, just barely. That first year, for some strange reason that nobody can explain to me, Roger Revelle decided to teach physical oceanography. He was a very interesting man. I liked him a lot, but he was a terrible teacher because he didn't understand, I think, some of the principles himself very well. He never would prepare a lecture beforehand, and he mumbled a lot. He stood in front of the blackboard, and he wrote with one hand and erased with the sleeve of his jacket of the other. Pretty much, you know, mystified us all. I didn't do well in that course, but it was through his approval that I was continued on as a graduate student. So he saw some potential in me, I guess.

PAUL DAYTON: Was this about 1953?

JOHN McGOWAN: Yeah.

PAUL DAYTON: And so you were scuba diving then too?

JOHN McGOWAN: Well, Connie Limbaugh had just started his first Course in diving and in aqua lung. We practiced in the swimming pool down in the La Jolla Beach and Tennis Club. They allowed us to use it, and our first ocean dive was in the kelp beds off of La Jolla.

PETER BRUEGGEMAN: You just did it because it was available, as a Course?

JOHN McGOWAN: Yeah.

PETER BRUEGGEMAN: You didn't know you were going to use it in Research?

JOHN McGOWAN: No, I had no idea. I was just interested in diving, looking at stuff under water like I did in the vernal ponds in Oshkosh, Wisconsin. It was the thing to do. I was a good swimmer. I learned to swim in Lake Winnebago, and done a lot of it, so it didn't bother me at all. I was halfway through my graduate studies when the word came out through the Director's Office that it would be nice if I applied for this position in Micronesia. Micronesia at the time was a UN trusteeship, and the United States was the governing body. This was a post World War II mechanism for taking care of former colonies of the Axis powers. Micronesia at the time, had been occupied by the Japanese for 20, 30 years, and they were, of course, forced out, so there needed to be a governorship of some sorts. First of all, the United States Navy set one up, and then the UN decided it should be demilitarized, not only Micronesia but everywhere else that they had trusteeships. The Department of Interior was the designated government agency to take it over. They set up an agriculture department, medical, legal, and administrative, and blah-blah. I must say, they did a pretty good job. They really were concerned about native welfare and getting things straightened out and improving their economy and educating, and getting good medical care. Of course their budget was next to nothing, and it was remote. Poor Micronesians had no vote, but never the less, on balance, I think it was done all right. One of the main sources of foreign exchange income for the islands was a pearl shell by the

name of Trochus niloticus. A big snail, big heavy pearly, a lot of thick nacreous shell. That shell had been harvested for generations and used in the button trade. All men's dress shirts had pearl shell buttons in those days. So, the harvesting of Trochus was well established even during Japanese times and before. The Japanese had actually transplanted Trochus from the western Carolines to the eastern Carolines in Micronesia, and there was Trochus harvesting done on those islands where the shell was not native. In 1954, or 1955, there were a lot of complaints that the Trochus harvest was falling off, diminishing. By the time the word got back to Washington, by the time it was verified locally, and then the word got back to Washington, there were serious problems with Trochus fishery. Reading between the lines, I think what happened was the Department of Interior sought out the advice of the National Academy of Sciences. One of the functions of the National Academy was to advise government agencies, and the National Academy put one of their members in charge, who was a Washington lawyer, and an activist for sure, and the grandson, or maybe it was the son of President Coolidge. Harold Coolidge his name was, and he was a very stuffy, proper, Washington bureaucrat, and New England, you know. He also had a degree in I think it was anthropology. He had scientific training. Well, Harold Coolidge contacted Roger Revelle. Scripps Institution knows everything at all, everything there is to know about the Pacific Ocean, and Micronesia is in the Pacific Ocean, and Roger could you find someone to go out there and do something about Trochus? Well, Roger, looked over the roster of students here, and there was John McGowan, who was single, without children, and he had done a lot of scuba work, and he was robust and healthy, and a good swimmer, and he worked on mollusks. It all fell perfectly in line, and he called Martin Johnson to tell me I was going. Of course, it wasn't quite that blunt, but that was the idea.

PETER BRUEGGEMAN: It was made obvious to you.

JOHN McGOWAN: Made pretty obvious that this was a really great opportunity. And actually, I looked forward to it. I didn't take much convincing. In the first place it paid pretty well, I was GS-11. In those days it was a good salary, and it was only for two years. It was diving on coral reefs, which I learned to love. So yeah, I did it.

PETER BRUEGGEMAN: Did you do scuba diving over there?

JOHN McGOWAN: Oh, yeah. I had a budget. I purchased an aqua lung and a compressor, which you could get in those days.

PAUL DAYTON: Cornelius compressor, single stroke? Pop, pop, pop. Knew it well.

JOHN McGOWAN: Yeah. Yeah. Yeah.

PAUL DAYTON: Just the same time I was using one.

JOHN McGOWAN: Yeah. Those damn things. I had trouble with that son of a gun. A fiberglass skiff and a Johnson 16-horse outboard. The Trochus was on the outer reef, a good long way from the islands.

PETER BRUEGGEMAN: On which island were you?

JOHN McGOWAN: Well, I was based in Palau, and that Trochus is native to Palau. In many ways that was the largest fishery. So the first thing I had to do was learn a little natural history. The literature on the topic was practically zilch, and half of it was in Japanese anyway. They had done some studies, pretty good ones. As a matter of fact, they introduced some catch regulations, the season and the size. It wasn't working, but they tried. It was par for the course in those days. You know, they did the right thing.

PETER BRUEGGEMAN: You hired somebody to motor around who knew the water so you wouldn't go aground?

JOHN McGOWAN: I hired two Palauan assistants. One was called Rick-Rick who was a diver and had worked for Americans before.

PETER BRUEGGEMAN: And you saw the Japanese planes in shallow water there.

JOHN McGOWAN: Yeah.

PETER BRUEGGEMAN: Did you go to Jellyfish lake or were you aware of it?

JOHN McGOWAN: Oh yeah, sure. I pretty much focused on the reef.

PAUL DAYTON: You owned a boat then?

JOHN McGOWAN: Yes. 16-foot fiberglass. We had it shipped out, with a 16-horse outboard engine. Rick-Rick new something about engines. He was a smart guy, and he spoke English. He and I collaborated on these studies. Curiously enough, he didn't know any arithmetic. He had trouble with long division, but he could add and subtract.

PETER BRUEGGEMAN: Palau is known for many things in diving. But also some of the drift diving along reefs where there is pretty good currents. Did you get diving into any of those environments.

JOHN McGOWAN: Oh, sure. That's where the Trochus live, there are pretty good currents.

PETER BRUEGGEMAN: So you would drop in and he would just drift along the surface in the boat? Or did you use a line with a float?

JOHN McGOWAN: We buddy dived together. We would anchor the boat. We would go to the outer edge of the reef. There is a big barrier reef, not the fringing reef, but the barrier reef outside where there was a lot of surf action. We anchored the boat at some convenient spot, and then make just transects, counting.

PETER BRUEGGEMAN: You had to deal with currents getting back to the boat? … there are some ripping currents.

JOHN McGOWAN: Yeah, there are some. Well, it is during a massive tidal exchange, I think. But mostly the reefs we looked at were because the best Trochus reefs were south of the southern sector of the barrier reef system. South of Koror. We did some transects up north of there, but they were a lot easier somehow. The reef was closer to the land.

PAUL DAYTON: Do you want to maybe summarize your idea on reserves that you set up in your final report, because I think it is really quite important.

JOHN McGOWAN: Yeah. Well, first of all, I got some catch statistics from the trading companies, and they kept track of how many pounds of shell were purchased by them, by each municipality. Most of the island groups were divided up into what we call municipalities, but they were villages that had so much territory, and that included reefs. So by sector, if you check with a local residents, their sector of the reef would begin here and end there. So we knew how much tons per mile of reef, and what were the good spots, and what were the bad spots.

These were catch statistics from the trading company, had little chits, pieces of paper. Rick-Rick and I, and Adolph that was the second Palauan, put all those together and made graphs. We did size-frequency studies, age-frequency studies, and all of that stuff, but there really wasn't enough data, and it was very confusing. We couldn't sample the catch as often as we wanted to for as long as we wanted to. So I tried to think of a way of adding to the already pretty good Japanese regulations. I suggested we keep them in place, keep the seasons and the size. No Trochus under a certain size. I think the size was a little too small, but never mind. What else could I do?

PAUL DAYTON: Well, the reserves, you had them so there were breeding areas, and they were harvested in the fringe.

JOHN McGOWAN: That's right. My conclusion was that one of the reasons the catch had been declining is that they thinned out the natural spatial pattern in places where there had been very little fishing. I had determined that Trochus tend to live together in groups.

PETER BRUEGGEMAN: Like abalone need to be close together to spawn successfully.

JOHN McGOWAN: Yeah, well, I am responsible for that. I told Mia Tegner this story. Trochus and abalone are highly related biologically… broadcast spawners. So, it was quite clear to me that these little patches had been thinned out dramatically, so I discussed it with some native leaders. I said, you ought to have segments of your reef set aside as no-take zones for anything, but I was specifically talking about Trochus because they need to get together and spawn, and they understood perfectly.

PETER BRUEGGEMAN: They knew it already?

JOHN McGOWAN: Well, I guess so.

PAUL DAYTON: Bob Johannes showed that they knew it for fish very well.

JOHN McGOWAN: Yeah. I set up patches within each municipality's reef zone. Then we figured out a marking method so that they wouldn't cheat and poach, and then reseeded these areas with groups of seven. I figured out with the sex ratio, you needed a minimum of seven shells in order to ensure both sexes are present.

PETER BRUEGGEMAN: So you weren't sexing the Trochus?

JOHN McGOWAN: You couldn't, they look identical on the outside. The only way to sex them is to drew a big hole in the shell and kill them to find out whether they had an ovary or testes, which were very different color. The ovaries are green and testes are grayish white.

PETER BRUEGGEMAN: And so there was good uptake on the idea by the community… they implemented it?

JOHN McGOWAN: Oh yeah, absolutely. No doubt about it.

PETER BRUEGGEMAN: But you were there such a short period you wouldn't see results?

JOHN McGOWAN: I didn't see the results, and no one in the trust territory bothered to tell me later. The Trochus industry declined precipitously because the rate of supply of our shells to the manufacturers of men's shirt buttons was so uncertain and so ephemeral that they all switched over to plastic. There is not much of a market for shirt button Trochus anymore. They still make jewelry, but that never was, is not today a very big consumer of shells, so I think the industry probably doesn't exist anymore. Incidentally, it was not just in Micronesia. It was in the Philippines and New Guinea and Australia and the Andaman Islands in the Indian Ocean, so it was widespread.

PAUL DAYTON: So roughly what were the dates for Micronesia?

JOHN McGOWAN: I made a preliminary trip out there in 1955. Then spent 1956, 1957, and part of 1958 there. Came back in 1958.

PAUL DAYTON: So before you went to Micronesia, you had published the first scuba reviewed paper anywhere. Do you want to talk about it just a bit?

JOHN McGOWAN: I was taking Connie's course in aqua lung. I had already started work on mollusks under Martin Johnson, and squid, of course, are mollusks. In addition to pteropods and heteropods, I got interested in squid, and I became the local cephalopod expert. Mostly through default, entirely due to default. One time, a fisherman came to the Scripps Director's Office and said he was a gillnet fisherman out of La Jolla shores here.

PETER BRUEGGEMAN: So they were launching boats from the shores at that time commercially?

JOHN McGOWAN: Yeah, and he said he couldn't haul his net up because he had all this stuff stuck to it. There were millions of squid eggs on the net. The gillnet had been anchored along the bottom --- this was near the head of the La Jolla submarine canyon. So I got a boat here and launched it from the pier. A diving buddy, Ivan somebody, who also had been taking Connie's course, and I went to head of La Jolla Canyon and dove down. I guess it was 50-feet or so, and there were millions of squid eggs spread all over the bottom. The eggs are in a big capsule, about eight to 10 inches long. And 2- or 300 eggs per capsule, a big white capsule, and a female squid will lay half a dozen of these, attach it to some object on the bottom.

PETER BRUEGGEMAN: And people at Scripps weren't aware of the squid spawning runs out there in La Jolla Canyon at that time?

JOHN McGOWAN: No. No.

PETER BRUEGGEMAN: Or local divers or anything?

JOHN McGOWAN: No, it was brand new information.

PAUL DAYTON: I remember they had the first aqua lungs, that was 1954, that's about when the first aqua lung came to the US.

PETER BRUEGGEMAN: Yeah, but Frank Haymaker was diving with a helmet for Francis Shepard.

JOHN McGOWAN: That's right.

PETER BRUEGGEMAN: And he was out in the La Jolla canyon too.

JOHN McGOWAN: That's right. Squid spawning is highly seasonal.

PETER BRUEGGEMAN: And it's not every year. So he could have been out there and it wouldn't have happened. So you just hit it right.

JOHN McGOWAN: I remember Francis Shepherd telling us about Haymaker. He didn't say much. So, there were lots of squid in the water mating.

PETER BRUEGGEMAN: So you were right in a big run.

JOHN McGOWAN: Yeah, we hit it just perfectly. And lots of sea birds, pelicans, and gulls.

PETER BRUEGGEMAN: Sea lions buzzing around.

JOHN McGOWAN: Sea lions.

PAUL DAYTON: Sharks.

JOHN McGOWAN: Sharks, and the squid littered the bottom. They died after spawning which is news. So I looked at all of this stuff and put some squid eggs in an experimental aquarium and counted the number of days it took to hatch and the number of eggs per egg capsule and blah-blah-blah, and assembled a bunch of information on my observation and submitted it to California Fish and Game, and that is where it was published.

PETER BRUEGGEMAN: That's the article where you talked about the Capitella worms in the squid egg capsules.

JOHN McGOWAN: That's right. They colonized those squid egg capsules. It doesn't take them long to discover.

PAUL DAYTON: Two weeks ago, one of our graduate students gave a seminar on that classics course, and featured your 1954 paper. The stuff that you talked about as being blah-blah, was very accurate, you actually measured a whole lot of things, and apparently you measured them much more accurately than several subsequent papers. Anyway, so that was before you went to Micronesia. It was right at the cusp of when aqua lungs came to the US.

JOHN McGOWAN: I think so.

PAUL DAYTON: It must have been almost the same summer that Connie taught the course. You got that paper out which has persisted as a classic because it was so good, if I might say so. I think that was pretty neat.

JOHN McGOWAN: Yeah. Thank you, it was fun. It was a lot of fun.

PAUL DAYTON: Then you went to Micronesia and lots of other adventures, and then you came home from Micronesia.

JOHN McGOWAN: Yeah, well, I finished my Ph.D. thesis in 1958.

PAUL DAYTON: What was your Ph.D. thesis on?

JOHN McGOWAN: Pretty dull. I had been assigned by Martin Johnson to look at mollusks in the California current, CalCOFI. Ed Brinton and Bob Berry and I shared a basement lab in old Ritter Hall. Ed was working on CalCOFI euphausiids. Berry was working on CalCOFI chaetognaths, and I was working on CalCOFI pteropods and heteropods, and looking at cruise after cruise of these patterns. It was primarily Bob Berry, but also Ed Brinton that began to realize that you couldn't really understand the spatial pattern and temporal changes of these organisms in the California Current without looking at a much larger scale. None of them were endemic to the California Current, they all being part of a much larger pattern, a geographic scale. We had been preconditioned to look at large scale stuff, I think by our studies of physical oceanography. Chiefly Roger Revelle and Bob Arthur who emphasized large scale processes are responsible for these physical patterns, water mass distribution. It was Sverdrup who originated that whole concept of looking at water mass patterns and identifying them and using mechanisms to quantitatively describe them. It turns out that the patterns we were seeing look remarkably like Sverdrup's water mass patterns. Berry and Brinton and I talked an awful lot about biogeographic realms; we didn't know quite what words to use. Then Bill Fager came along and started giving formal lectures about community, and community structure, and community interactions. It became pretty clear to me at least, that these biogeographic species patterns were in the first place replicated between different groups, chaetognaths, euphasiids, copepods, pteropods. They all had, I guess you would call it redundant patterns. You could see chaetognaths, copepods, and euphasiids, and those patterns look like Sverdrup's water masses. It occurred to me that these were community ecosystems, and the California Current is just part of a larger system, and changes in the California Current were part of a much larger scale processes going on. Then, at the same time, Revelle was talking about global change and carbon dioxide, so that they were super-imposed on these time series of the lesser frequency. There was this very large scale process going on, of physical change, and it seemed to be little question that that must have had its biological consequences. But getting enough large scale samples, spatial scale and then temporal scale, to determine that took a lot of doing, an awful lot of doing. A lot of counting and a lot of examination, and a lot of uncertainty.

PAUL DAYTON: You published a paper with Fager early on, on biogeographic realms.

JOHN McGOWAN: That's right.

PAUL DAYTON: With some brand new statistics.

JOHN McGOWAN: Yes, it took me a long time to figure out what the hell he was talking about, but he was very helpful. The question that he tried to address was quantitatively define and describe the word community. Lots of biologists, ecologists, up until that time felt that species associations are just chance affairs, that there is no reason to believe that there is any kind of obligate relationship between the groupings of the assemblages of species that you find in samples. Fager made the point, which seemed to me to be very clear and unambiguous, but vague to a lot of people, I guess, that if as a matter of fact, species do tend to live together, that they should have an influence on one another's life, population, and what we need to do is define what we mean by together. Is this an obligate thing, or is it just chance? Well, you have a bunch of samples taken over time and space, and you can ask the question, do certain pairs of species tend to occur together more often than you can attribute to chance? Then you can ask whether all possible pairs in your set of samples occur together more often than chance alone? The way to do this is just count presence and absence. Then, if you find that there are groupings of species, recurrent groups he called them, that occur together more often than chance alone, dictate, you can do a further analysis to see whether or not there is any agreement within the group as to variations and abundance. If you start out with looking at variations and abundance, that's say cross correlation between a bunch of species, you run the risk of having a predator prey species that are oscillating. One down, the other's up, and they have a negative correlation. Well, you throw that out as evidence for co-occurrence, when as a matter of fact, that is exactly what is happening, one depending on the other. They both depend on one another for their dynamic, and you need to find out first whether they co-occur frequently, and then look at the population variations in time and space. So it is a two-step process and the critics of the Fager approach, never understood that. Gee, if you just used presence and absence, don't you miss a lot? Well, hell yeah, you do, you are only halfway there. By the time we figured this out, I figured out what he was talking about, Brinton, and Berry and I, and Tom Bowen had done a lot of biogeographic patterns for the whole North Pacific, several thousand samples. I went to Fager, and I said look, I think we can do some kind of community structure here. That's when he explained to me the regroup method. He said, "Yes, as a

matter of fact" and he whipped out this paper that he done in Oxford. So we spent the next year or two putting the data together and writing that paper of zooplankton regroup in the North Pacific.

PAUL DAYTON: It was 1963, I think.

JOHN McGOWAN: There has been nothing since then but affirmation, validation. We had it right.

PAUL DAYTON: Another paper from as I recall, 1963, which was, I think you went back to Oregon State for some meeting, and they asked you to publish something in a book. And in 1965, I read it and was much impressed by it. I didn't read the Fager one because I couldn't understand it.

JOHN McGOWAN: Yeah, difficult.

PAUL DAYTON: But I could understand your speciation paper, a little thing you did in 1963 on planktonic oceanic speciation.

JOHN McGOWAN: Yeah.

PAUL DAYTON: It still boggles my mind.

JOHN McGOWAN: Well, there is a problem with standard explanation for a speciation mechanism in the ocean. There is a problem in the open ocean because of so much mixing. The mechanism for speciation, the standard one in most textbooks in evolution and ecology, is that there needs to be some sort of spatial, or maybe behavioral, but usually spatial separation of populations in order for enough sexual isolation to build up genetically during sexual isolation to build up so that they are isolated from one another, should they come back together again at some future date. I don't see how that could possibly be true in the plankton because there is so much mixing along species boundaries. You don't find populations isolated from one another. All of the species that occur in the Atlantic are also in the Pacific, and there is exchange between the oceans, quite a bit of exchange. There is enough recent exchange anyways so that and many species patterns, separate species pattern within the Pacific that simply can't be explained by any spatial isolating mechanism. So there needs to be some sort of a rethinking of the possibility of how sexual isolation and that is genetic separation, incompatible crossing can occur. I came up with this one possibility in Limacina helicina, because on the basis of the distribution of varieties, it looks like one of them is

constantly being selected for in this current system, and that there is a filter. While they are still interbreeding, they are really quite morphologically quite different. Anyway, the problem wasn't solved by that paper, but --

PAUL DAYTON: It was the first thing that I had seen that -- and I don't know that literature, but that asked the question --

JOHN McGOWAN: Well, we did it again later on. Pat Walker and I did it with copepods. There are a lot of recurrent groups of copepods that contain congeners that are very much like one another, and they are not separated out spatially or temporally. They occur together. Even in a finely divided vertical pattern, they don't separate out in any sample size we could devise. Incidentally, later on, Tom Hayward did a wonderful study of the feeding of those copepods to show that they feed at the same time and place. They do not behaviorally separate by feeding, and they eat the same thing.

PAUL DAYTON: So the beat goes on with that question.

JOHN McGOWAN: Yeah.

PAUL DAYTON: It is a damn interesting question. The thing is you were identifying it and writing about it really almost as a graduate student. It would have been '61, '62.

JOHN McGOWAN: I read Ernst Myers book at that time. Not only that Bochanski Dobzhansky whom I much prefer. He writes beautifully. Like a lot of people whose second language is English, he can write.

PAUL DAYTON: Yes, that was the world of the early '60s. They were asking interesting questions. The thing is that you would have been a student back from tthree years in Micronesia. You did your thesis, but you were at the same time asking classical questions of speciation in the ocean that haven't been answered yet. You did that paper with Fager, which has to be a landmark in my mind.

JOHN McGOWAN: Well, thanks to him.

PAUL DAYTON: And all in the early 1960s.

JOHN McGOWAN: Yeah. That's right.

PAUL DAYTON: You also worked on some of the most interesting pelagic organisms that I know of, the pteropods, the heteropods. Then because of maybe your early 1954 effort, you have never lost your interest in squid. You've published some important papers on squid taxonomy, and heteropods, and pteropods.

JOHN McGOWAN: Well, pteropods, little pelagic mollusks that have beautiful shells, and they also provide fossils for bottom sediments. They are mostly considered sort of cute, which they are, but they are not important in any trophodynamic sense, because they are simply never abundant enough to process much energy. They are not a big factor or segment of any food way. They are wide spread and part of the overall biogeographic pattern and rather easy to identify and count, because they do differ strongly from one another in their shell shape and dimension. What is interesting about them quite apart from their ecological role is that they are derived from tetrabranch mollusks, from nudibranchs and opisthobranch mollusks. They have a lot of the same morphological features except that they have shells. They have retained their larval shells and developed into much more elaborate things. They have little flaps that they use for swimming, and those flaps are the edges, judging from their innervations their nerve endings, the edges of the nudibranch mantle. Mantle ledges have grown out to become swimming wings. Now, the heteropods are a different story. They are derived from a different branches of gastropods, and they have retained their shells too. They use an entirely different method for developing swimming ability. They have a fin, and there is a sucker on it. Nobody knows what that sucker is for, but, nevertheless, there it is. They undulate this fin in a nice figure eight like sculling, and they can move pretty rapidly. They have a big long nose on them that has not only a radula like all mollusks, like all gastropods, but it has seizing jaws. They are voracious as hell, vicious little predators.

PAUL DAYTON: How did you watch all of the behavior in the 1960s?

JOHN McGOWAN: Well, you go to sea, and you take a net tow, and you put stuff in a bowl, and look at it under a microscope for about 20 minutes before it dies. We tried to keep some alive in an aquarium for maybe a few hours.

PAUL DAYTON: Do you ever get that other (planktonic) gastropod, I've forgotten its name, that pretty little purple snail.

JOHN McGOWAN: Oh Janthina, it's a proper snail, but it lives only on the surface, floating on the surface tension. A lot of organisms have countershading coloration, where their bellies are lighter than their backs. It is reversed in Janthina, because they float upside down with their mouths and with the shell up. Beautiful dark blue there in the tropics. They can bloom, great masses of them can all of a sudden appear. It is suspected that they may eat ctenophores and jellies, surface living.

PAUL DAYTON: Do you have anything else that you think would be interesting?

JOHN McGOWAN: Well, I had a certain amount of what I think is success teaching students. I have had 21 Ph.D.'s. I would say the majority have gone on to become academics and functional producing scientists. I have lost track of maybe three or four, but the others and I are still good friends. I made certain that their thesis work was pretty much independent. Of course, it had to be within my field, that's why they chose me as a chairman, but I never directed their research program, they picked it themselves. I stressed freedom to explore, independence. I insisted when they publish their thesis, they do it independently, without my name. I have collaborated with a number of them since then. One of these collaborations was one paper that I think is very important. That is Stommel's diagram paper about time, space, relationships.

PAUL DAYTON: Why don't you tell the history of that diagram, because it is quite famous and very important paper.

JOHN McGOWAN: Yeah, I've been plagiarized wonderfully, extensively.

PAUL DAYTON: Which is an honor.

JOHN McGOWAN: I guess, especially if they cite you, which most of them do not. I had been lecturing for some time to my class in pelagic ecology, on the different scales of variability, and time, and space and talking about these as they were separate problems. That they were fine scale, small scale patches and bigger and bigger and bigger finding out the scale of biogeography. Also, that there were ephemeral blooms, and these were also at a bunch of different scales. These were all in my mind kind of separate lectures. Along about that time, and it was I guess, let's say 1962 or so, Stommel published a paper on variations in sea level heights in which he showed that they occurred on different time space scales, but these time space scales were related. If you drew a diagram of time versus space, you see

this lumping looking contour. It was a three dimensional diagram since heights, was a third dimension that came out of the page. I talked about that too. I didn't put it together with my lectures on time and space scales and plankton. And Patricio Bernal came up to me one day, and he said, you have got the same thing. Why don't you do that, make one of those? Patricio was a graduate student at the time. Well, at the time, I had been invited to give a talk at one of those NATO symposiums. Peter Wiebe and Loren Haury and I were putting together this talk. I discussed it with Peter and Loren. Loren discussed it with some physical oceanographer, whose name I have forgotten, and between the two of them and I, we made this diagram, time and space diagram of variability with zooplankton for which there were data. Of course, we gilded some of the data and interpolated in between gaps and all of that stuff, which everyone seems to do. It turns out that there is a relationship between time and space variability, that really big changes in abundance tend to be large scale. We started with centimeter scale and worked all the way up to global scales, and the time was minutes to palatial periods, time and space. That was accompanied by the table of questions relating to the diagram, more or less in the realm of, so what? There are these changes. What does that mean? That table is usually ignored. People like the diagram, and there are many versions of it. The important thing about our version is that it is three dimensional, not just two. I think it has something to do with the notion that a lot of ecological events and processes are long-term and large scale. In our diagram, the long-term large scale events stick out of the page, they are bigger than a lot of small scale stuff. If you persist on studying ecology on the scale of meters and days, you are never going to find out – you can't resolve the large scale changes in time or space. You need a long time series and a large spatial distribution of samples to detect variations on that scale. Variations on that scale are really big, and therefore important.

PAUL DAYTON: I think it's a great paper, and you mentioned students. I have often told people that from my perspective, the field of biological oceanography has been pretty much written by you and your students. And I am a bit prejudiced perhaps but --

JOHN McGOWAN: Yeah, sure you are.

PAUL DAYTON: -- I think they have had a huge impact in the field.

JOHN McGOWAN: Well, I never felt that trophodynamics is a very interesting or important field. Much of biological oceanography has to do with something called trophodynamics. You have these highly stylized trophic levels which themselves

are extremely dubious, in my opinion. Then energy transfer, or maybe nitrogen transfer, the rate of transfer of energy through trophic levels and efficiencies, and so-called dynamics. I think that concept has dominated biological oceanography, unfortunately. Community, structure, ecosystem, dynamics, biogeography, all of that stuff is not all that fashionable in my field. Never was, and still isn't.

PAUL DAYTON: Well, maybe my view is prejudiced by my interest in those questions….

JOHN McGOWAN: Yeah. Sure.

PAUL DAYTON: More than the ecosystem issues. But in my view, I would say there is a huge impact. Maybe your post-docs, people like some of the Japanese with whom you have interacted. Your name is really associated indirectly through much of squid taxonomy, for example.

JOHN McGOWAN: Yeah.

PAUL DAYTON: And many of them have been your students.

JOHN McGOWAN: Due to Takashi Okatani.

PAUL DAYTON: And John Wormoth. And so, there are these little footprints all over the ocean.

JOHN McGOWAN: Well, that's students, and that's one of the power sources at Scripps. We really have good students, and of course I think they are treated well. They get good training, not necessarily from you and me, but the faculty in general.

PAUL DAYTON: Yeah, a very collegial place for students.

JOHN McGOWAN: It was. I'm worried about that in the future. I think we are growing farther and farther apart. The administration is responsible for a lot of that.

PAUL DAYTON: The students, though still interact, I think, fairly well with --

JOHN McGOWAN: With one or another. Yes, I hope so.

PAUL DAYTON: Yeah. Do you want to look through your notes and see if there is something else that you wanted to talk about?

JOHN McGOWAN: Well, yeah. I am interested now and have been for some time about what I call comparative ecosystem structure, comparing one part of the ocean to another in terms of rank or abundance. I use rank because it is an easy way of standardizing. The ability to do that came mainly through Bill Fager who had developed methods of looking at proportions of things present and how that changes with time, and how you can compare one set of time space measurements with another set of time space measurements. Doing it statistically and simply so you can ask questions,… does the central gyre of the North Pacific, which after all contain all the components of plankton systems, does that system, is it structured the same way over time as say the California Current? The answer is hell, no. They all have copepods and chaetognaths and chlorophyll and nutrients, but the arrangement of these things with respect to one another, the rank order of these things is really quite different in the two systems. Yet they both persist over time, they are not going to hell. Then we also have compared North Pacific central gyre to the South Pacific central gyre, which are similar systems in many ways. They are oligotrophic and warm and ancient. My God, they are about some of the oldest ecosystems on earth. They have the same species in them. However, the rank order of abundance is not the name. Some of the dominants in the north are not dominants in the south, and vice versa. And yet, both systems are equally stable over time, and there is no exchange between them. Big super gigantic central equatorial water mass separates them. So they are faunistically separate but faunistically similar. Identical in terms of species, but not in the order of dominance. What that, among other things, says is that resilience and persistence, doesn't really depend on who is boss and who isn't. The dominants in the north are different than the dominants in the south. Not all of them, some of them are the same. It is not critical if there is some missing, or some things that are abundant that become rare. Hell, there are abundant species in the north that are very rare in the south.

PAUL DAYTON: Yeah, I regret the fact that many of these questions are no longer considered interesting.

JOHN McGOWAN: Yeah. Well, they are hard to do. You can't get samples like that very often.

PAUL DAYTON: Yeah. Maybe that's the problem.

JOHN McGOWAN: Yeah. That's part of it.

PETER BRUEGGEMAN: So, before you went off to Micronesia, you were diving just to explore around here in La Jolla.

JOHN McGOWAN: Yeah, I didn't do all that much of it. Just a few dives in the kelp beds.

PETER BRUEGGEMAN: Just a few?

JOHN McGOWAN: Well, it was expensive, and the truth is, for a half hour or 45 minute dive, it takes an awful lot of time. Getting ready and getting the boat and cleaning up after. It is a whole day. A whole damn day.

PETER BRUEGGEMAN: So you were trying to understand it as a tool in addition for science. And it sounds like you are going, this is very limited in what one could possibly do in terms of --

JOHN McGOWAN: I can't say that it was fun, and I wanted to have fun. Yeah. A huge big investment.

PETER BRUEGGEMAN: Where as you can go to the tropics and dive like crazy and get data --

PAUL DAYTON: All day long, yeah.

PETER BRUEGGEMAN: Right. So that's the hard way to get data, it sounds like you had an element in understanding that.

JOHN McGOWAN: Yeah. And after a while in Micronesia, we just did skin diving. We didn't even use scuba. Didn't need to for what we had to do. We didn't dive that deep. Dove all day. Got lots of transecs done. It was a simple matter just to put on face plates and a little breathing tube.

PETER BRUEGGEMAN: When you came back to Scripps, did you do any diving with Scripps? Or you were already onto other things then?

JOHN McGOWAN: Onto other things, and the water here is so cold and so dirty. It was so much trouble. There were so many rules and regulations. The hell with this.

PAUL DAYTON: While you were talking, I was thinking. I went to graduate school in 1965 and also wanted to be a marine biologist all my life. It was very hard in the 1960s to get a grounding of marine biology. There wasn't much. That is why I found your speciation paper, for example. I was desperate for some sort of guidance, some sort of mental framework that I could put my career around. I sort of had to make my own. You were here at Scripps down there in what is now the director's office, the business office area. There was a lot of marine biology going through Scripps. World-class marine biologists, were here. You had the diving program with the deep sea thing. Astronauts were here. You had that Italian of geneticist who wrote a book.

JOHN McGOWAN: Buzzati-Traverso.

PAUL DAYTON: Yeah, who wrote a book. There was a book on marine biology that had things like Edmundson's paper. You know, important papers. That's all we had somewhere else to get a figure of what marine biology was, but you had it all right here beside you.

JOHN McGOWAN: That's true. Fager had a lot to do with it. He provided me with some kind of ability to quantitatively describe what it was that I was interested in, and do it in a simple, straight forward, unambiguous way. No question of it. He was very important.

PAUL DAYTON: Cousteau was here, with his saucer. You know, there was all sorts of things were going on.

JOHN McGOWAN: John Isaacs was here. He had a lot to do with it.

PAUL DAYTON: And he had his DUKW. And he had Bascom.

JOHN McGOWAN: Willard Bascom. Yeah. That's another thing. There were all these bomb tests going on, not only at Bikini, but in the semi-tropical water south of Baja California, there is a big under water test, very important test. And of course plankton was a big part of it.

PAUL DAYTON: Anyway, I just wondered if you might have some parting comments on what the era was like in the '60s. It must have been really exciting.

JOHN McGOWAN: Yeah. Well, there was Joel Hedgpeth at his peak. I didn't feel that it was all that exciting at the time. It was just what was going on. I didn't know any better. Oceanography was booming. Scripps was building buildings, huge big grants, and CalCOFI was going to beat hell. Half a dozen other programs too, that's not the only one. An awful lot being done. Revelle had a lot to do with that. He was a very impressive guy, and a real intellectual.

PAUL DAYTON: Yeah, and John Strickland and Benny Schaefer.

JOHN McGOWAN: Yeah, that's right. They all had strong influence in Washington, and we had strong influence within the state of California. Isaacs pointed out that one time many of the state legislators were graduates of Berkeley and UCLA, but that was changing rapidly. They are many graduates of state colleges now, San Diego State, San Jose State. The University is much less important to them.

PAUL DAYTON: I think we have covered the things that I've thought of pretty well.

JOHN McGOWAN: Okay.